Review of "**Changes in seismic anisotropy at Ontake volcano: a tale of two eruptions"** by Kendall et al.

Overview and general recommendation:

This work aims to demonstrate the ability of seismic anisotropy to differentiate stress changes around two eruptions of different magnitude. It links volcanic processes with one of their geophysical responses, clarifying how different stress levels can be flagged using seismic observations.

The work follows within the Scope of Seismica and is of value and interest to a significant portion of the potential readers of the journal. It is clear and easy to follow, short and factual, with a good abstract and title. The implications of the work are significant for the volcano seismology community. The methods are well-established, with several case studies demonstrating their efficiency in volcanic contexts. The dataset used is outstanding. The conclusions are adequate and supported by the data. The study does not propose results significantly different from academic consensus. The results of Okada's methods strengthen the conclusions.

My critical comments focus on the study's methods, presentation, the possibility of reducing the number of figures, and the data/software open data policy. If the previous and following comments can be satisfactorily addressed by changes in the manuscript or the response letter, I will be happy to support the publication.

Please beware that the references used to explain myself do not need to be cited.

Comments:

1) Technical

a) Anisotropy measurements have been established as a reliable measurement to monitor and image volcanic unrest. You correctly identify (L131-137) the mechanisms producing anisotropy and recognise stress anisotropy as the primary contributor to anisotropy within the crust. It might look pedantic, but clarifying the difference between crack and stress anisotropy (if any) might be necessary for the volcano seismology community. Can pre-existing cracks cause anisotropy without further stress at the levels observed?
b) You ask the reader to compare Figures 6 and 4 to prove their core conclusions. This is difficult. The figures show the same observations with different time resolutions, so I understand the choice of showing the second over a temporal axis. However, comparing parameters over time with those on a map, having their length corresponding to per cent anisotropy, does not provide enough validation for your claim. The obvious solution is to produce both plots (temporal and on a map for both periods), even if the second would comprise a much smaller area.
c) Even if not as important, something similar can be said for Figs. S1 and S2. Plotting them on the same axis would let the readers better appreciate the comparison.
d) Transverse anisotropy is not ideal when describing volcanic systems. Could you add a paragraph in the intro or discussion where orthorhombic (monoclinic) symmetries are discussed, explaining why applying them is challenging (impossible)?
e) Also, why not include a plot like Figure 5 for 2014?
f) The paper needs to mention open data/software availability statements: this is a significant shortcoming for Seismica.

2) Interpretation and presentation

a) Why can't the differences in anisotropy percentage be entirely caused by the differences in earthquake depths? You discuss this in the text, but maybe a more explicit statement negating this possibility would be better. This comment does not contradict the temporal changes before and after the eruption.
b) L421. Maybe it is better to mention here the numerous theoretical and field studies that have established that *it is host rock stresses that dictate magma pathways*, as here it seems the opposite: During ascent to the surface, the dikes align themselves with the most energy-efficient orientation, which is roughly perpendicular to the least compressive principal stress axis (e.g., Dahm, 2000; Maccaferri et al., 2010). Contrary to intuition, preexisting faults appear of subordinate importance in guiding magma (Anderson, 1951), as their orientation relative to the stress field is optimised for shearing movements; thus, opening along such planes is inefficient. This does not negate the paper's assumptions, but the volcano community is picky about these details (I talk from experience). It also supports the conclusions.
c) There are essential differences in earthquakes' time, depths and spatial distribution between the 2007 and 2014 datasets. Discussing the uncertainties these bring to interpreting the results will clarify how confident you and the readers might be about the interpretation.
d) Apart from Figs. 4 and 6, Fig. 1 could easily be implemented as a panel in Fig. 2.
e) Figs. 2 and 3 will be compared; this could be done better in the same figure. Also, having different symbols for stations and topography in the two figures is confusing.
f) Some names of stations in Fig. 2 are unreadable. As it is not essential for the paper, you could remove the names, leaving only station V.ONTA in the first (the one common to both periods).
g) Fig. 6 could be stretched to better appreciate the variations near the eruption in both panels. Alternatively, what about a zoom as a sub-panel?

3) Minor

L36-39 It took me a while to understand the meaning of the particle "but" in the second sentence. Then I realised what you mean is that, despite average anisotropy rising already at the start of 2014, there is a much more significant increase after it. Correct? If so, as they are written, it is unclear; maybe add an "*after it*" at the end.

L94 Add a comma after "*Here*".

L106 – "… *provide**s*** …"

L125 – What do you mean with "*fluid changes*"? Migration and storage? Changes in saturation? Can you be more specific?

I wonder why the work of Caudron et al. (2022) has not been cited in the introduction when discussing fluids contributing to anisotropy: it seems relevant both technically and for the discussion.

Best Wishes,

Luca De Siena

References

E. M. Anderson, *The Dynamics of Faulting and Dyke Formation with Applications to Britain* (Oliver and Boyd, ed. 2, 1951).

Caudron, C., Aoki, Y., Lecocq, T. et al. Hidden pressurised fluids prior to the 2014 phreatic eruption at Mt Ontake. Nat Commun 13, 6145 (2022). https://doi.org/10.1038/s41467-022-32252-w.

T. Dahm, Numerical simulations of the propagation path and the arrest of fluid-filled fractures in the Earth. *Geophys. J. Int.* **141**, 623–638 (2000).

F. Maccaferri, M. Bonafede, E. Rivalta, A numerical model of dyke propagation in layered elastic media. *Geophys. J. Int.* **180**, 1107–1123 (2010).

Review: “Changes in seismic anisotropy at Ontake volcano: a tale of two eruptions” by J. M. Kendall et at.

In this manuscript, the Authors measure S-wave splitting parameters, recorded by a network of seismic stations close to the top of the Ontake volcano (Japan), in two separate time-windows: before and after the 2007 eruption, and before and after the 2014 eruption (larger than the 2007 eruption). The measured parameters indicate a variation of the splitting of S-wave from local events during the 2014 eruption, but not during the 2007 eruption. The changes in splitting parameters are interpreted as changes of the rock anisotropy, which turns out to be a proxy for the state of the local fracture network and its variations in time. The Authors suggest that such change in anisotropy can be given by either a change in the fluid filling the cracks (from gas to liquid), or in an increase of liquid-filled cracks. The results of the study confirms that S-wave splitting analysis can be a monitoring tool for large eruptions.

Overall, I found the description of the data and data processing very well detailed. Moreover, the findings are statistically consistent. I suggest to improve the presentation in some specific points, which I will list below, to make it more readable to a wide audience.

A. Comparison of 2007 and 2014. At line 434, the Authors ask the reader to compare Figures 4 and 6. However, such comparison is really difficult due to the different representations (a map in Figure 4 and two time-series in Figure 6). Also, for a statistical comparison, it becomes necessary to read the results reported in Table S1 for station ONTA, i. e. the mean and the STD for the splitting parameters before and after the 2007 eruption. Even if only one event is analysed after the 2007 eruption for ONTA, it seems to be statistically reasonable a “no-changes" scenario across such eruption. Conversely, from Figure 6, the reader can easily observe the changes during the 2014 eruption period. Please, make an Ad Hoc Figure for the comparison of the two periods.

Minor modifications:

Abstract. Include definition of VEI (volcanic explosivity index (VEI))

Lines 143-145. "The time separation or delay time (dt) between the arrival of the two shear waves is an indicator of the strength of the anisotropy and the spatial extent of the anisotropic region.” In my view, this description is misleading. Splitting parameters measure the integral of the rock anisotropy along the ray-path, and not the 3D spatial distribution itself. I understood what the authors mean, but I would clarify the phrase.

Line 244. It seems that the seismic network was in place before the first VEI2 eruption in 1979 (?) Do we have data for that? Or any publication?

Figure 2. Station names are barely visible. I think it is better to remove them from the figure and: either add a table with names and coordinates or add a panel with only station names. The label for V.ONTA should be kept in the main panel.

Figure 3. Please, could you add a panel showing the same area but for the 2007 Eruption, to better understand the event distributions in the two periods? Thanks.

Figure 4. What do the dark yellow lines close to the top of Ontake indicate?

Figure 6. Please, can you split the two panels in four sub panels, two panels for each station? It helps the readers.

Figure 6. Add STD for ONTA in 2007, from Table S1, in all panels

Figure 6 (and in general). Stations are mentioned with the Network Code and without ("V.ONTA" and "ONTA"). Please, choose one and use throughout the full text, also in Figures and figure captions.

Response to comments from the handling editor and the reviewer:
"Changes in seismic anisotropy at Ontake volcano: a tale of two eruptions", Kendall et al., submitted to Seismica.

We thank the editor and the reviewer for their insightful comments and suggestions. We have modified the paper accordingly and feel that their advice has improved the clarity of the paper. Below in black are the comments from the review. *Blue, italics show our responses.* And blue normal font show what new text has been added to the paper. A version of the paper with tracked changes shows the changes we have made.

Handling editor comments:

I suggest to improve the presentation in some specific points, which I will list below, to make it more readable to a wide audience.

A. Comparison of 2007 and 2014. At line 434, the Authors ask the reader to compare Figures 4 and 6. However, such comparison is really difficult due to the different representations (a map in Figure 4 and two time-series in Figure 6).

This is a very good point. We felt that it was not worth making a new map to show the temporal variation in the 2014 sequence, as it would be two very closely spaced data points. Instead, we produced a rose diagram for the two stations considered in the 2014 data. These clearly show the variability in the fast directions. But, to enable comparison, we have added the 2007 results for the station V.ONTA to the rose diagrams for 2014 to make a new Figure 5. We feel that this better shows the constancy of the 2007 results and the variability of the 2014 results. We have also directed the readers to compare Figures 4 and 5 with Figure 6.

B. Also, for a statistical comparison, it becomes necessary to read the results reported in Table S1 for station ONTA, i. e. the mean and the STD for the splitting parameters before and after the 2007 eruption. Even if only one event is analysed after the 2007 eruption for ONTA, it seems to be statistically reasonable a “no-changes" scenario across such eruption. Conversely, from Figure 6, the reader can easily observe the changes during the 2014 eruption period. Please, make an Ad Hoc Figure for the comparison of the two periods.

A Figure with dt (or percent anisotropy) as a function of time for only V.ONTA does not really convey much, as there are few data points. Instead, we have plotted the average dt and the standard error for the 2007 measurements on Figure 6. This is also in accordance with the editor’s suggestion below.

Minor modifications:

Abstract. Include definition of VEI (volcanic explosivity index (VEI))

Done

Lines 143-145. "The time separation or delay time (dt) between the arrival of the two shear waves is an indicator of the strength of the anisotropy and the spatial extent of the anisotropic region." In my view, this description is misleading. Splitting parameters measure the integral of the rock anisotropy along the ray-path, and not the 3D spatial distribution itself. I understood what the authors mean, but I would clarify the phrase.

We have changed this sentence to: … provides an integration of the effect of anisotropy along the entire raypaths, which is a function of the strength of the anisotropy and how it varies over the region sampled.

Line 244. It seems that the seismic network was in place before the first VEI2 eruption in 1979 (?) Do we have data for that? Or any publication?

Sadly, we do not have access to the seismic data from the earlier time periods. Japanese seismic data is owned by many organisations and often difficult to access. Fortunately, these data have been made more open in recent times, but we do not know how to access the legacy data (if any exists).

Figure 2. Station names are barely visible. I think it is better to remove them from the figure and: either add a table with names and coordinates or add a panel with only station names. The label for V.ONTA should be kept in the main panel.

We have simply removed Figure 2, as it showed much more detail than is needed for the paper. Instead, we have modified what was Figure 3 to show the events and stations used for both the 2007 and 2014 sequences.

Figure 3. Please, could you add a panel showing the same area but for the 2007 Eruption, to better understand the event distributions in the two periods? Thanks.

Done, as explained above.

Figure 4. What do the dark yellow lines close to the top of Ontake indicate?

They indicate locations of erupted material, and we have added this clarification to the figure caption (Now Figure 3).

Figure 6. Please, can you split the two panels in four sub panels, two panels for each station? It helps the readers.

We have left the two panel figure in the main text, but added the four panel figure, as suggested, to the SI (Figure S2). Please let us know if you would rather have the 4 panel figure in the main text. The coverages of 2 stations are within a Fresnel zone at the depths of these earthquakes.

Figure 6. Add STD for ONTA in 2007, from Table S1, in all panels

Done

Figure 6 (and in general). Stations are mentioned with the Network Code and without ("V.ONTA" and "ONTA"). Please, choose one and use throughout the full text, also in Figures and figure captions.

We have used V.ONTA and V.ONTN throughout.

Response to comments of Reviewer B:

Technical

Anisotropy measurements have been established as a reliable measurement to monitor and image volcanic unrest. You correctly identify (L131-137) the mechanisms producing anisotropy and recognise stress anisotropy as the primary contributor to anisotropy within the crust. It might look pedantic, but clarifying the difference between crack and stress anisotropy (if any) might be necessary for the volcano seismology community. Can pre-existing cracks cause anisotropy without further stress at the levels observed?

An interesting comment. People have a tendency to interchange the two terms, and there is some ambiguity in the term stress-induced seismic anisotropy. From a theoretical point of view, pre-stress leads to anisotropy in the higher-order elastic constants (in other words, Hooke's law breaks down and higher-order elastic constants need to be developed – see Dahlen, BSSA, 1972). But this affect is subtle. In the shallow crust, a much more dominant mechanism is associated with sub-seismic-wavelength cracks and fractures. In an isotropic stress field, it is reasonably assumed that microcracks will be randomly aligned. Stress anisotropy will close microcracks in the direction perpendicular to the direction of maximum horizontal stress. However, microcracks can be also aligned due to previous deformation events. There is also the ambiguity of microcracks versus fractures. Fracture have a tendency to align in conjugate sets, bisected by the direction of maximum horizontal compressive stress – indeed in previous work we had sufficient data coverage to invert for two sets. This is not the case in this study – we have a limited number of measurements at each station. So for simplicity, we assume a single set of microcracks or fractures aligned with the stress field.

We have added the following text at line 154 in the revised manuscript:
There is some ambiguity in the term stress-induced seismic anisotropy. From a theoretical point of view, pre-stress leads to anisotropy in the higher-order elastic constants (in other words, Hooke's law breaks down and higher-order elastic constants become important – see Dahlen, 1972), but this affect is subtle. In the shallow crust, a much more dominant mechanism is associated with sub-seismic-wavelength cracks and fractures. Stress anisotropy will close microcracks in the direction perpendicular to the direction of maximum horizontal stress. However, there is also the ambiguity in the use of the terms microcracks versus fractures. Fractures tend to occur in conjugate sets, resulting in a fast shear wave polarisation that is some weighted average of the two orientations (e.g., Verdon et al., 2009). As we have a limited number of measurements at each station, we assume for simplicity a single set of microcracks or fractures aligned with the stress field.

You ask the reader to compare Figures 6 and 4 to prove their core conclusions. This is difficult. The figures show the same observations with different time resolutions, so I understand the choice of showing the second over a temporal axis. However, comparing parameters over time with those on a map, having their length corresponding to per cent anisotropy, does not provide enough validation for your claim. The obvious solution is to produce both plots (temporal and on a map for both periods), even if the second would comprise a much smaller area.

To address the request for a map, we have produced rose diagrams for the 2014 sequence for both V.ONTA and V.ONTN (now in Figure 5). We don't feel that plotting these symbols on a map adds much, and the rose diagrams add detail. For further clarity, we have superimposed the rose diagrams for V.ONTA on the 2014 results. This makes it much easier to make a direct comparison. In Figure 6, we have added the average V.ONTA time delay and standard deviation for the 2007 measurements.

Even if not as important, something similar can be said for Figs. S1 and S2. Plotting them on the same axis would let the readers better appreciate the comparison.

Done. We have produced a new Figure S1.

Transverse anisotropy is not ideal when describing volcanic systems. Could you add a paragraph in the intro or discussion where orthorhombic (monoclinic) symmetries are discussed, explaining why applying them is challenging (impossible)?

This is a good point. In reality there is likely some sub-horizontal layering or alignment that leads to a VTI fabric (e.g., Kendall et al., 2007). The stress-induced crack anisotropy is reasonably assumed to have an HTI symmetry. The two fabrics combine to produce an orthorhombic symmetry (or monoclinic if the beds are dipping). However, an important point is that we are using sub-vertical raypaths (to stay within the shear-wave window). Therefore, the rays are not sensitive to the VTI component of such fabric. There is a possibility of a more general form of anisotropy due to an azimuthal *alignment of crystals. But the temporal variation in anisotropy that we see cannot be attributed to a crystal preferred orientation. The background anisotropy could be of a more general form, but the fact that it extends no deeper than 5km, again suggests that a crack mechanism is dominant.*

At line 144 in the revised manuscript, we add:
Here we assume stress-induced crack anisotropy is the dominant mechanism for anisotropy in a volcanic setting. Vertically aligned fractures produce a horizontal transverse isotropy (HTI) symmetry (i.e., a rotational invariance in seismic velocities around a horizontal symmetry axes) (see Kendall (2000) for more detail). In reality there is likely some sub-horizontal CPO, layering or alignment that would lead to a vertical transverse isotropy (VTI) fabric. The two fabrics combine to produce an orthorhombic symmetry (or monoclinic if the beds are dipping) (e.g., Kendall et al., 2007). However, an important point is that we are using sub-vertical raypaths from earthquakes that lie beneath a seismic station (to stay within the shear-wave window). Therefore, the rays are not sensitive to the VTI component of such fabric.

At line 550 in the interpretation section, we add:
We note that there is a possibility of a more general form of anisotropy due to an azimuthal alignment of crystals, which may explain the spatial variation in anisotropy. But the temporal variation in anisotropy that we see cannot be attributed to a crystal preferred orientation (CPO) mechanism. In general, the background anisotropy could be of a more general form, but the fact that it extends no deeper than 5km, again suggests that a stress-controlled crack mechanism is dominant.

Also, why not include a plot like Figure 5 for 2014?

We have done this.

The paper needs to mention open data/software availability statements: this is a significant shortcoming for Seismica.

Agreed – Data are archived on Zenodo – DOI: 10.5281/zenodo.14598534.

Interpretation and presentation

Why can't the differences in anisotropy percentage be entirely caused by the differences in earthquake depths? You discuss this in the text, but maybe a more explicit statement negating this possibility would be better. This comment does not contradict the temporal changes before and after the eruption.

To explain the differences in anisotropy through differences in depth would be difficult. In general, the 2007 delay time increases with depth, until ~5km (Figure S1). In 2014, the events are shallower and show larger delay times than the comparable depths in 2007 (also Figure S1), which means larger anisotropy magnitude in 2014. A possible *explanation would be two layers of anisotropy with fast directions oriented orthogonally. But this is more complicated and seems an unlikely explanation.*

L421. Maybe it is better to mention here the numerous theoretical and field studies that have established that *it is host rock stresses that dictate magma pathways*, as here it seems the opposite: During ascent to the surface, the dikes align themselves with the most energy-efficient orientation, which is roughly perpendicular to the least compressive principal stress axis (e.g., Dahm, 2000; Maccaferri et al., 2010). Contrary to intuition, preexisting faults appear of subordinate importance in guiding magma (Anderson, 1951), as their orientation relative to the stress field is optimised for shearing movements; thus, opening along such planes is inefficient. This does not negate the paper's assumptions, but the volcano community is picky about these details (I talk from experience). It also supports the conclusions.

This is an interesting comment. There should be a trade-off where the host rock stress dictates magma pathways, but magma injection will also locally perturb the stress field. This is what the modelling (Figure 8) shows. Regarding the second point, the role of preexisting fractures and faults is of great debate in oil and gas systems, and there is no clear consensus. In volcanic systems it may be a question of scale – a dyke intrusion will likely follow the direction of maximum horizontal compressive stress, but fluids in the hydrothermal system may follow pre-existing weaknesses. However, it is difficult to imagine the temporally varying signal we observe not being controlled by stress changes.

At line 510 we have added:
During ascent to the surface, dykes align themselves with the most energy-efficient orientation, which is roughly parallel to the direction of maximum compressive stress (e.g., Dahm, 2000; Maccaferri et al., 2010). However, the movement of magma through dyke emplacement will locally modify the stress field.

There are essential differences in earthquakes' time, depths and spatial distribution between the 2007 and 2014 datasets. Discussing the uncertainties these bring to interpreting the results will clarify how confident you and the readers might be about the interpretation.

Need to add something about more data needed for the wider distribution of events.

Apart from Figs. 4 and 6, Fig. 1 could easily be implemented as a panel in Fig. 2.

For simplicity, we elected to keep the Figure as is.

Figs. 2 and 3 will be compared; this could be done better in the same figure. Also, having different symbols for stations and topography in the two figures is confusing.

These figures have been harmonised and combined in a two-part Figure 2.

Some names of stations in Fig. 2 are unreadable. As it is not essential for the paper, you could remove the names, leaving only station V.ONTA in the first (the one common to both periods).

Agree – we have modified the Figure, as described above.

Fig. 6 could be stretched to better appreciate the variations near the eruption in both panels. Alternatively, what about a zoom as a sub-panel?

We feel that the new Figure S2 shows this quite clearly.

Minor

L36-39 It took me a while to understand the meaning of the particle "but" in the second sentence. Then I realised what you mean is that, despite average anisotropy rising already at the start of 2014, there is a much more significant increase after it. Correct? If so, as they are written, it is unclear; maybe add an "*after it*" at the end.

No, this is not what we meant. The highest rise in anisotropy is at the onset of the 2014 eruption. The delay time doubles, but the events are much shallower, so the percent anisotropy rises more dramatically than delay time. We have added: …. but because the events shallow in depth the percent anisotropy increases dramatically from 3% to 20%.

L94 Add a comma after "*Here*".

Done – thanks.

L106 – "… _*provides* …"

Done – thanks.

L125 – What do you mean with "*fluid changes*"? Migration and storage? Changes in saturation? Can you be more specific?

We went back to the original Wegler et al. (2006) paper – they interpret velocity changes as an indication of fluid pressure changes. We have added the word "pressure" to the text.

I wonder why the work of Caudron et al. (2022) has not been cited in the introduction when discussing fluids contributing to anisotropy: it seems relevant both technically and for the discussion.

This is an oversight, and we thank the reviewer for catching this omission of what is a very relevant paper. We have added:

Line 125: Subtle changes in seismic velocity during eruption have been interpreted in terms of fluid or pore pressure changes (Wegler et al., 2006; Caudron et al., 2022).

*Line 252**:*** Caudron et al. (2022) observed a sequence of correlated seismic velocity and volumetric strain changes starting 5 months before the eruption.

Line 545: On a longer time scale, Caudron et al. (2022) observed changes in seismic velocities, which they attributed to changes in volumetric strain occurring in the period of 5 months before the eruption.
